a time when they undergo such rapid extensions, should form the crowning part of chemical studies, and the interdependence of the two branches of science can only be established upon a sound basis when a thorough knowledge of either science has been acquired.

The main portion of the book comprising the chemistry of the non-metallic and metallic elements, arranged under the usual headings Preparation and Properties of the different elements and their compounds, contains much that will highly recommend itself. The more important compounds are dealt with in a manner which will help the student over most of the difficulties he encounters at first, and will enable him to lay a good foundation for more extensive chemical studies. The classification of the metals according to their atomicity—open to objections as it stands—is not always reconcilable with the analytical summaries or tests given after each group of metals, nor are the analytical explanations always accurate. On p. 393, *e.g.* we notice: "Calcic sulphate cannot produce a precipitate in a salt of calcium, because there is more than enough of water present to retain dissolved all the sulphate that can possibly be formed."

There can be little doubt that the new edition of Wilson's Chemistry will be welcomed by all who desire to get a general insight into the science, and that it may be studied with advantage in preference to many larger and more ambitious text-books.

OUR BOOK SHELF

Verhandlungen der k. k. geologischen Reichsanstalt. Nos. 11 to 18. (Vienna.)

THESE numbers of the Proceedings of the Geological Society contain many useful papers, chiefly, however, of local interest. Felix Karrer notes the occurrence of mammoth remains at Vienna. They were obtained during the sinking of a well in a "diluvial" (glacial) deposit at a depth of 9 fath. 3 ft. from the surface. Dr. Lenz also has a short reference to a similar discovery of the teeth of a young mammoth in a brown laminated loam near Nowakmühle. Dr. Stur gives an interesting account of his own and Baron Petrino's observations on the superficial deposits in the basin of the Dniester in Galicia and Bukowina. A great accumulation of loess covers a wide area in that district, the land shells and mammalian remains in which enable these geologists to correlate it with the loess of the Rhine and other regions. We observe, in No. 11, an important table showing Dr. C. E. Weis's systematic arrangement of the carboniferous formation and the rothliegendes formation of the Saar-Rhine district, which is well worth the attention of English geologists. The usual admirable literary notices, and other miscellaneous matter, are appended to each number of the Proceedings.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Leaf-Arrangement

THE chief part of the Rev. G. Henslow's objections (NATURE, vol. vii., p. 403) to my condensation-theory of leaf-arrangement are due to a double oversight on his part. First, he has overlooked the condition of *contact* among the balls which I use to represent embryo leaves. Second, he has overlooked the fundamental position, that leaf-order exists for, and is determined in, the *bud*.

The bud requires economy of space. This involves contact among the embryo leaves; and if we experiment with balls attached (as described in my paper) in two rows alternately on either side of a contractile axis, we shall see that when the axis is allowed to contract with a twist, that twist is necessarily limited by the conditions of contact which arise, and that we cannot "cause it to make a complete rotation if we choose." Given the size of the balls and their distance from the axis, the position which they will assume (under contraction with a twist) is necessarily determined by the geometrical conditions of mutual contact. This consideration furnishes the answer to Mr. Henslow's italicised query, and also to his two previous questions (1) and (2). It also gives back a "really explanatory meaning" to my expression "*maxima* of stability," for if we have one sphere standing almost vertically on another and supported by a third and a fourth to right and left, we have therein some statical conditions which admit of maxima and minima of stability. The same consideration also removes the objection that "the positions taken up by the balls must be arbitrary, or at least in proportion to the twist given by the hand—a perfectly arbitrary force." The twist given by the hand in my experiment serves only to determine the direction of the real twist; the subsequent real availing twist is insisted upon by the two ranks of balls-in-contact as the sole condition of obedience to the contractile force of the indiarubber axis; and this twist is limited by the conditions of contact above described. Let the direction of the twist be given, and there is nothing arbitrary in the result.

The objection that "if an axis becomes twisted the *fibres* will be twisted also" loses force if we bear in mind that the leaf-order is imposed upon the embryo leaves in the very earliest stage of their bud-life; and that the formation of fibres, taking place at a subsequent stage, must find itself compromised by an already existing arrangement of the embryo leaves. The elastic band in my experiment, "if it were a pliant shoot," would certainly "contort the vessels and wood-fibres;" but it was not meant to represent a "pliant shoot" except in its earliest embryonic bud-stage, and that at some very remote period in the past.

I must ask Mr. Henslow to bear in mind that he has before him only the abstract of my paper, and that necessary brevity has left some points too bare, and has wholly suppressed others of small importance. Among the latter is some mention of the "secondary series" $\frac{1}{3}$, $\frac{1}{4}$, $\frac{2}{7}$, &c., which, though it may be found in the abnormal variations exhibited by a cultivated plant like the Jerusalem Artichoke, yet cannot be reckoned with examples of normal leaf-order.

Let me take this opportunity of insisting again on the astonishing agreement between the facts of nature and the results which the condensation-theory leads us to expect. Taking one member to start from as 0, we find in nature that the members in contact with 0 belong to the following series, 1, 2, 3, 5, 8, 13, 21, 34, 55, 89, 144, &c, and these are the very same members which would necessarily be brought into contact with 0 under successive degrees of condensation with twist from an original order $\frac{1}{2}$.

I have lately met with a striking confirmation of the truth of the condensation-theory. The simplest order of the whorled type is that in which the leaves stand in pairs, decussate. Now if we consider what would be the result of condensation with twist applied to this arrangement, we can see that it would produce a new series of orders, in which the following members would successively come into contact with 0,—2, 4, 6, 10, 16, 26, 42, &c., and would present the phenomenon of 2, 4, 6, 10, &c., spirals alternately to right and left. This result is exemplified in nature. The teazle (*Dipsacus silvestris*) has the decussate order in its leaves; and in its head (where we might expect to find its leaf-order condensed) we count sixteen conspicuous spirals in one direction and twenty-six in the other:—that is to say, we have 0 in contact with No. 16 on one side and No. 26 on the other. No. 42 stands higher between 16 and 26, but inclined towards the former. No. 10 stands next below 26, and No. 6 next below 16. These numbers belong to the new series above mentioned.

This close parallel between fact and theory appears to me to give a value to the latter which it will not lightly lose.

March 30 HUBERT AIRY

The Hegelian Calculus

As Dr. J. H. Stirling has enjoyed the exceptional privilege of replying contemporaneously to my paper on Hegel in the

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current number of the *Fortnightly*, I should desire, with your kind permission, to find in your columns the opportunity of saying without delay the single word which still seems necessary between Dr. Stirling and myself.

Dr. Stirling now holds that the real question between him and me is whether or not Hegel "attempted" to produce "a Hegelian Calculus." And so it seems to him a virtual concession of the entire case when I say that the phrase "Hegelian Calculus" is used by me in irony. Dr. Stirling, I fear, misunderstands me. What Hegel has given us on the subject of the Calculus is, strictly speaking, nonsense. But, as I have shown, this nonsense is not mere metaphysic, but involves mathematical absurdity. It is of course only in irony that one can dignify the paradoxes of mathematical ignorance with the title of a Calculus; and if this admission satisfies Dr. Stirling, then our controversy is at an end. W. Robertson Smith

Aberdeen, April 3

Meteorology of the Future

I wish to call the attention of the writer of the article "The Meteorology of the Future," which appeared in Nature of December 12, 1872, to a little work which appears to have entirely escaped his notice.

In the beginning of 1871 I circulated a small book of twenty-four pages, containing results deduced from the observations made at this Observatory, 1841 to 1870. I have given the decimal and annual variations of all the meteorological elements collected, and have pointed out their mutual interdependence. I have also given on an enlarged scale the curves of variations of annual mean temperature and freedom of the sun's disc from spots, which appeared in the Proceedings of the Royal Society, March 23, 1871.

No one acquainted with the subject would, I presume, believe that periodical variations could exist in the temperature without existing also in the other meteorological elements; vapour as measured by tension, hence barometer humidity and rainfall.

In the introduction to the work referred to, it is stated with regard to the curve of temperature and inverse curve of solar spots:—"There is an agreement between the curves which will probably be regarded as too close to be the result of accident, and which renders it probable that the two phenomena, represented by the curves, result from the action of a common cause connected with changes of mean solar energy." And this established with more or less probability, I proceeded to point out (p. 17)—"That the variations of temperature are borne out by those of tension of vapour," and on page 22—"That the correspondence between humidity and rainfall is strongly marked," and also that—"The correspondence between a curve swept to represent the variations in rainfall and the inverse curve of the variations in mean temperature is of a marked character."

You will perceive, therefore, that the connection between solar spots as an indication of less solar heating power and vapour, and rain, as well as temperature, was in the book referred to explicitly pointed out. I may add to this note, that the rainfall for

1871 was 20·098 inches
1872 was 29 325 inches.

E. J. Stone

Royal Observatory, Cape of Good Hope

Bright Meteor

I have this evening, at 7.40, seen the brightest meteor I have ever beheld: starting from a point about half-way between Cassiopæa and the Pole star, it descended through about 20° of arc, when it was lost sight of behind a cloud: this cloud was a thick white opaque cloud shining brightly in the moonlight, but the meteor behind it illuminated the sky, and made the cloud appear for the moment dark against it.

The colour of the meteor was a decided green; its passage was not very rapid; it appeared far brighter than any star or planet, and seemed to have a short tail. Not only was it a gloriously beautiful object in itself, but it illuminated all the sky in its neighbourhood with its greenish light.

Edmund H. Verney, Commander R.N.

H.M.S. *Growler*, off Cape Matapan, March 5

The Great Meteoric Shower of November 27, 1872

This interesting display was also observed in the neighbourhood of the small town of Santa Lucia in Venezuela (10° 12′ N., 68° 57′ W. from Paris), by Dr. A. Alamo. The first meteors were seen at half-past 7, about 100 in 30 minutes. Most of them followed an easterly course, some leaving a luminous track visible for several minutes. From 8 to 12 o'clock their number was too large for counting, but after midnight the weather got misty, and few meteors could be distinguished. The shower, however, continued, and still in the morning some meteors were traceable. Unfortunately Dr. Alamo cannot say anything about the radiant point of the shower. At Carácas the sky was densely overcast, and not even a glimpse of the spectacle could be obtained. Dr. A. Ernst

Carácas, Feb. 21

The Antiquity of Man

The letter of Sir John Lubbock in your issue of March 27, induces me to call attention to what seems to me to be an anomaly in the state of our evidence concerning fossil man. Sir J. Lubbock has insisted, and with much reason, on the parallelism between the condition of existing savage races and that of fossil man; but, I would ask, is there any existing savage race capable of delineating animals in the masterly way in which the elephant is delineated on the plate of bone figured at page 326 of Nature (February 27, 1873)? Such a life-like representation as is here produced by a few rough scratches would not discredit a modern artist. Unless I am under a misapprehension, the best figures that living savages can produce are but uncouth things, in which case either the parallelism between the intelligence of existing savage races and of fossil man fails in one important particular, or else a suspicion arises as to the contemporaneity of these engraved bones with palæolithic man; and a doubt is thrown on the supposed antiquity of the Troglodytes to whose hands this engraving is ascribed.

We should, I think, until this discrepancy is explained, look with still greater suspicion upon the contemporaneity of engraved representations of animals with so early a form as Miocene man, or accept them as any evidence of his existence at that epoch.

While suggesting the above caution, I would not, however, be understood to dissent from the probability of some form of man having existed as far back as the Miocene period, since eleven years ago, I observed in the *Phil. Mag* (for April. 1862, last paragraph but one of the paper) that the views there discussed "seemed to me to lead us to the presumption of a far greater antiquity for our race than had hitherto been accorded to it, reaching perhaps far back into the Tertiary period."

Brentwood, Essex Searles V. Wood, Jun.

Skeletons at Mentone

A very accomplished geologist, a friend of mine, is now staying at Mentone, for the benefit of his health, and he writes to me under the date of the 25th ult. as follows:

"Another skeleton has just been found here in one of the caverns. It is far less perfect than the former one. The head is crushed and partly wanting, and a considerable portion of the vertebral column is absent. The limbs, however, indicate a person of larger size than the first skeleton. On the arms are bracelets of shells, which are bored for stringing. The parts found are lying in their natural position With the skeleton are traces of what looks like very fine iron ore. Of this substance there is but a very small quantity, perhaps two or three tablespoonfuls."

With regard to the iron ore, there have been many conjectures, and it is extremely remarkable that about the same quantity of a similar substance was found with the first skeleton. The more general opinion seems to be, that this material was employed in some burial rite. W. T.

Torquay, April 1

[From a cutting from *Les Echos de Cannes* sent us by W. T., we learn further that the head was covered by a network of shells, and that beside the skeleton were found many implements of bone, and even drawings of fish and swans.—Ed.]

Instinct

Perception in Ants

The following fact with respect to the habits of ants, which I believe to be quite new, has been sent to me by a distinguished geologist, Mr. J. D. Hague; and it appears well worth publishing. Charles Darwin

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